Biogeosciences Discuss., 12, C4871–C4873, 2015
www.biogeosciences-discuss.net/12/C4871/2015/

Biogeosciences
Discussions
Open Access

Interactive comment on "Application of clustering techniques to study environmental characteristics of microbialite-bearing aquatic systems" *by* R. Dalinana et al.

Anonymous Referee #2

Received and published: 31 August 2015

The study evaluates several pore water chemistry datasets in the literature from locations of modern microbialite growth using statistical methods. By means of a cluster analysis the authors attempt to reveal the common factors affecting microbialite formation. Based on the results, pH and salinity are ruled out as controlling factors.

While I am unable to judge the statistical methods, I see major shortcomings in the application of these methods:

In the present version of this manuscript it is not clearly defined which process is being considered. The formation of microbialites involves several fundamentally different processes, each of them responding to different factors. In a first step, a microbial

biofilm or microbial mat may grow to a variety of shapes and sizes. Only a minority of microbial mats is in fact calcifying and results in the formation of a hard lithified microbialite. The lithification process may also occur via different mechanisms (microbially induced vs. microbially catalysed precipitation). A distinction between processes would be fundamental for this study. For example, microbial mat growth may respond to competitive advantages with other organisms, or to the availability of organic matter substrates, a redox gradient, light intensity or inorganic macro- or micronutrients. In contrast, microbially induced precipitation depends on carbonate supersaturation (as a result of pH, alkalinity, DIC, Ca and Mg concentration). Mixing these two processes is like comparing apples to pairs. Correlations are invalid as they can be assigned to the wrong processes.

Probably the most severe flaw is the confusion between cause and consequence of the investigated parameters. For example, pH is often more strongly altered by the microbial community than it varies in the surrounding environment. Therefore the main output of this study, the pH not being a controlling factor, is meaningless, since the pH is the result and not the cause of the process. The same is the case for several other parameters, such as alkalinity, DIC, Ca and all other metabolites within the diverse microbial communities.

A further really fundamental shortcoming is the lack of a negative control. For a meaningful evaluation of the data, the authors should include datasets from sites showing microbial mat formation but lacking any calcification or microbialite growth. It should not be difficult to find such datasets, as non-calcifying mats are the majority. A possible study site would be Laguna Figueroa (Baja California).

The study does include positive controls with abiotic microbialites. But the question is again, which process is being considered. These control sites will certainly resemble the microbialite sites in terms of increased saturation state. Thereby I suggest comparing saturation indices (SI = log IAP – log Ksp) between all of the sites. In terms of microbial growth conditions (nutrients etc.) the microbialite sites are likely rather

different from the control sites.

A further question is, whether abiotic and microbially influenced microbialites can be really distinguished. We may think that microbial mats sometimes just become calcified, without actually contributing to their calcification. Such mechanisms have been recently suggested by Castro-Contreras for Laguna Bacalar (Mexico) or by Birgel et al. for Lagoa Salgada (Brasil).

P. 7, Line 25: Consumption of CO2 by photosynthesis does not affect the alkalinity. It increases the pH and decreases the DIC content, resulting in an increase in saturation state with respect to calcium carbonate.

P. 9, Line 15: Microbial carbonates that are not microbialites? This is not consistent with the definition of "microbialite" given in the introduction: "Organosedimentary deposits formed by trapping and binding or by microbially induced precipitation of minerals" (Burns and Moore, 1987). Is there an example of a microbial carbonate that is not a microbialite? Besides, it would be interesting to evaluate the cluster analysis in terms of type of microbialite (e.g. stromatolites vs. thrombolites, etc.).

The study lacks some essential literature references: A whole suite of papers has been published by Shiraishi et al. and Bissett et al. on microsensor measurements from the Deinschwanger and Westerhöfer Bach. These studies show very convincingly that precipitation is induced by the phototrophic action within cyanobacterial biofilms.

Interactive comment on Biogeosciences Discuss., 12, 10511, 2015.